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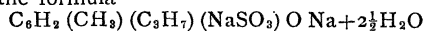
structure of the region where the copper occurs in northern Texas and the Indian Territory. He had received specimens from that region long ago and recognized their similarity to the copper ores of New Mexico, where in the upper portion of the Triassic formation copper forming concretions and replacing wood occur in many localities, and have been more or less mined for. In one locality near Abiquini very extensive galleries have been cut in the sandstone in search of copper which there replaces branches and trunks of trees and forms concretions which are irregularly scattered through the rock. Here the work was done by the early Spanish explorers perhaps 200 years ago, and the remains of the furnaces in which the copper was smelted are still to be seen at the mouth of the mine. Still further west, in southern Utah, the same formation carries copper and considerable silver, at Silver Reef enough to pay well for mining, but in no locality yet known are the deposits of copper ore sufficiently concentrated and continuous to make mining for that material profitable, so it would doubtless be found in Texas and the Indian Territory. The copper was deposited with the Triassic rocks from a shallow sea in which an unusual quantity of copper was held in solution. This impregnated the sediments found at the bottom replacing wood and forming as nodules about some nucleus. The aggregate quantity of copper in this formation was enormous, but, except where by the erosion of the beds it accumulated at the surface and could be picked up without any expense in mining, it would hardly pay to attempt to obtain it by ordinary mining processes.

The wood replaced by copper Dr. Newberry said was undoubtedly all coniferous, and different from any now living. The beds which contained the cuprified wood also contained much that was silicified. Of this he had examined many specimens under the microscope and had found the peculiar dotted cells which are characteristic of the coniferæ, and these grouped in such a way as to prove the trees to have belonged to the Araucarian group of conifers. So far as yet known the angiosperms, or higher order of plants, did not make their appearance on the earth's surface until after the copper bearing rocks of the southwest had been deposited.

#### THE AMERICAN CHEMICAL SOCIETY.

The November meeting of this Society was held on Friday evening, November 4th, with Vice-President Leeds in the Chair.

The following gentlemen were duly elected members: Dr. C. W. Volney, Dr. Witthaus, Messrs. C. E. Munsell, W. W. Share, J. D. O'Connor, and Day. The first paper of the evening was "On some New Salts of Thymole Sulpho-acid, and some new facts concerning the same," (a second paper) by Mr. J. H. Stebbins, Jr., S. B. The sodium salt having the formula



was described, and also the free sulphur salt had its characteristics enumerated.

Mr. Stebbins followed with a second paper "On the Combination of Diazo Compounds with Thymole Sulpho-Acid."

In this he described the experiments which he performed in his work, the results of which were given in the first paper. Both were technical and not of any popular interest.

The third paper was by Dr. C. W. Volney, and was entitled, "The Constitution of the Explosive Derivatives of Glycerine."

In this communication the author tried to prove that the nitro-glycerine was composed by the substitution of the nitrogen trioxide ( $\text{NO}_3$ ) instead of the nitrous oxide  $\text{NO}_2$ , making the formula  $\text{C}_3\text{H}_5(\text{NO}_3)_3$  instead of  $\text{C}_3\text{H}_5(\text{NO}_2)_3$ , and secondly, he showed how it was possible to substi-

tute chlorine for the nitrogen trioxide and so produce a new explosive compound.

This paper provoked much discussion on account of the theoretical arrangement of the atoms necessary to sustain Dr. Volney's statement.

Subsequently the Committee on Nominations reported that the following ticket was recommended to the Society for their votes at the December meeting.

*Corresponding Secretary*.—P. Casamajor.

*Recording Secretary*.—J. H. Stebbins, Jr.

*Treasurer*.—M. Alsberg.

*Librarian*.—Geo. A. Prochazka.

*Curators*.—A. J. Rossi, Wm. Rupp, A. A. Fesquet.

*Committee on Publications*.—Arno Behr, A. R. Ledoux, H. Endemann.

*Committee on Nominations*.—A. H. Elliott, O. H. Krause, J. P. Battershall, J. B. F. Herrishoff, T. O'C. Sloane.

*Board of Directors*.—P. Casamajor, J. H. Stebbins, Jr., H. Morton, C. F. Chandler, M. Alsberg, E. R. Squibb, W. H. Nichols, W. H. Habershaw, E. Waller, A. H. Galatin, Geo. A. Prochazka.

#### ON THE NATURE OF THE DIPHTHERITIC CONTAGIUM.

BY DR. H. C. WOOD.

The lecturer began by stating that the researches which formed the basis of the present address had been made under the auspices, and, indeed, at the suggestion, of the National Board of Health, by Dr. Henry F. Formad and himself, who were jointly responsible for the facts and inductions and jointly deserving of whatever reprobation or approbation might be due. The full text of the work is now in the hands of the National Board, and will be shortly published by them as an appendix to their annual report, and the lecturer desired that criticism be withheld until this was done, as the memoir will contain much that cannot be spoken of in the present lecture.

In the spring of 1880 work was begun by inoculating rabbits with diphtheritic membrane taken from the throats of patients at Philadelphia. An account of the labors of the following summer has been already published, but it seems necessary to epitomize them here. It was found that only in a very few cases was anything like diphtheria produced in the rabbit by inoculating with the membrane. The inoculations were practised by putting pieces of the material sometimes under the skin, sometimes deep in the muscles. Many rabbits died after some weeks, not of diphtheria, but of tuberculosis. In a series of experiments it was shown that this tuberculosis was an indirect and not a direct result of the inoculation, and that any apparent relation between the two diseases is only apparent, not real. Next, the tracheas of a series of rabbits were opened and false membrane inserted. It was found that under these circumstances a severe trachitis was frequently produced, and was attended by an abundant formation of pseudo-membrane. Careful studies made of the false membrane of diphtheria and of this false membrane showed that the two were identical, both containing in abundance fibrin fibres, corpuscular elements, and various forms of micrococci. To determine whether other inflammations of the trachea than that caused by diphtheria or its membrane are accompanied by the formation of false membrane, a number of experiments were made, and it was demonstrated that the production of false membrane has nothing specific in it, but that any trachitis of sufficient severity is accompanied by this product. Careful studies also showed that this false membrane does not differ in its constitution from that of true diphtheria, except it be that the micrococci are not so abundant in it. We always found some micrococci, and in some of these traumatic pseudo-mem-

\*AN ADDRESS MADE BEFORE THE ACADEMY OF NATURAL SCIENCES.

branes they were almost as numerous as in the diphtheritic exudation.

Last spring we resumed our investigations. Having heard that there was a very severe epidemic in Ludington, Mich., Dr. Formad was dispatched to examine cases and collect material. He found a small town situated upon the shore of Lake Michigan, in the centre of the lumber region, with inhabitants mostly engaged in the lumber trade and in managing very numerous large saw-mills. The town was all built upon high ground except the Third Ward. This occupied a low swamp which had been filled in largely with sawdust. The soil was so moist that a hole dug in it would fill at once with water, and but few houses had any attempts at cellars. It was in this district that the disease had prevailed. Almost all the children had had it, and one-third of them were said to have died. Dr. Formad examined a large number of cases, obtained a supply of diphtheritic membrane, and brought home pieces of the internal organs of a child upon whom he had made an autopsy. In every case the blood was found more or less full of micrococci, some free, others in zooglœa masses, others in the white blood-corpuscles. The organs brought home also all contained micrococci, which were especially abundant in the kidneys, where they formed numerous thrombi, choking up and distending the blood-vessels. In the summer of 1880 we examined the blood of several cases of endemic Philadelphia diphtheria, and in no case found any new elements in it. But during the present summer we have found micrococci in the blood of Philadelphia diphtheritic patients, showing the differences in the disease are simply in degree, not in kind.

Experiments were now made with the Ludington material upon animals. Inoculations were practised under the skin, deep in the muscles, and in the trachea. In all cases the results were similar. A grayish exudation appeared at the seat of inoculation, along with much local inflammation, the animal sickened, and in the course of a few days death occurred. The local symptoms increased and widened. In some cases the false membrane spread from where the poison had been put in the trachea up to the mouth. The blood examined during life or after death was found to contain micrococci precisely similar to those found in the Ludington cases, and in a few instances micrococci were found in abundance in the internal organs. Studies made upon the blood of these animals, as well as upon the Ludington cases, show that the micrococci first attack the white blood-corpuscles, in which they move with a vibratile motion. Under their influence the corpuscles alter their appearances, losing their granulations. They finally become full of the micrococci, which now are quiescent, and increase until the corpuscle bursts and the contents escape as an irregular, transparent mass full of micrococci, and form the so-called zooglœa masses. In the diphtheritic membrane the micrococci exist frequently in balls, and it is plain that these collections are merely leucocytes full of the plant. The bone-marrow of the animals were found full of leucocytes and cells containing micrococci.

The question now arose, is the disease produced by diphtheritic inoculation in the rabbit diphtheria? We concluded that it is, because the poison producing it is the same, the symptoms manifested during life are the same, and the post-mortem lesions are identical. The contagious character of the disease is retained, as we succeeded in passing it from rabbit to rabbit.

Our next series of experiments were directed to determining whether the micrococci are or are not the cause of the affection. The experiments of Curtis and Satterthwaite, of New York, have shown that the infectious character of diphtheria depends upon its solid particles; for when they filtered an infusion of the membrane it became less and less toxic in proportion as the filtration was more and more perfect; and when the infusion

was filtered through clay, the filtrate was harmless.

The urine of patients suffering from malignant diphtheria is full of micrococci, and may contain no other solid material. Following the experiments of Letzerich, we filtered this urine and then dried the filter-paper. Upon experiment we found this even more deadly in its effects than is the membrane. The symptoms and lesions following in the rabbit inoculation with such paper are precisely those which would have ensued had a piece of diphtheritic kidney or membrane been employed. This experiment shows that the solid particles of the membrane, which are the essential poison of malignant diphtheria, are the micrococci, which must be either the poison itself or the carriers or producers of the poison.

Leaving for a while this point, I will next direct your attention to our culture-experiments. These were performed in the manner commenced by Klein and that recommended by Sternberg. The first method seems to us the best for the purpose of studying the development of the micrococcus itself; the second the best for the obtaining of it in quantity for experimentation.

We cultivated micrococci from the surface of ordinary sore throats, from furred tongue, from cases of mild diphtheria as we commonly see it in Philadelphia and from Ludington cases. We found, in the first place, that there were no differences to be detected in the general or special appearance of the various micrococci, and no constant differences in size. We found that they all formed similar shapes in the culture apparatus; they had this difference, however,—whilst the Ludington micrococci grew most rapidly and eagerly generation after generation up to the tenth, those from Philadelphia diphtheria ceased their growth in the fourth or fifth generation, whilst those taken from furred tongue never got beyond the third transplantation. Various culture-fluids were used, but the results were identical. We conclude, therefore, that as no difference is detectable between the micrococci found in ordinary sore throat and those of diphtheria, save only in their reproductive activity, they are the same organisms in different states. As the result of some hundreds of cultures, we believe that the vitality of the micrococci under artificial culture is in direct proportion to the contagious powers of the membrane from which they have been taken. We have made many inoculations with cultivated micrococci and have succeeded in producing diphtheria with the second generation, but never with any later product. This success, taken in conjunction with the urine experiments already spoken of, seems to us sufficient to establish the fact that the micrococci are the *fons et origo mali* of diphtheria. The experiments of Pasteur and others have proven that it is possible for an inert organism to be changed into one possessed of most virulent activity, or *vice versa*, and we believe that we can offer direct proof that the micrococci of the mouth are really identical in species with the micrococci of diphtheria, and do not merely seem to be so. We exposed the Ludington membrane for some weeks to the air in a dried condition. There was no putridity or other change detectable in it; but, whereas formerly it had been most virulent, now it was inert, and its micrococci not only looked like those taken from an ordinary angina, but acted like them. They were not dead, they had still power of multiplication, but they no longer grew in the culture-fluid beyond the third or fourth generation. Certainly they were specifically the same as they had been, and certainly therefore the power of rapid growth in culture-fluids and in the body of the rabbit is not a specific character of the diphtheria micrococcus.

As is well known, Pasteur attributes the change from an active to an inert organism to the influence of the oxygen of the air upon the organism. Whether this be true or not of the diphtheria micrococcus is uncertain, but the effects of exposure of the dried membrane seem to point in such direction.

With the facts that are known in regard to the clinical history of diphtheria and those which we have determined in our research, it is easy to make out a theory of the disease which reconciles all existing differences of opinion and seems to be true.

A child gets a catarrhal angina or trachitis. Under the stimulation of the inflammation products the inert micrococci in the mouth begin to grow; and, if the conditions be favorable, the sluggish plant may be finally transformed into an active organism, and a self-generated diphtheria results. It may be, however, that by appropriate treatment such a case is arrested before it fairly passes the bounds of an ordinary sore throat. Every practitioner knows that such diversity does exist. Again, conditions outside of the body favoring the passage of inert into active micrococci may exist, and the air at last become well loaded with organisms, which, alighting upon the tender throats of children, may begin to grow and themselves produce violent angina, trachitis, and finally fatal diphtheria.

In the first instance we have endemic diphtheria as we see it in Philadelphia; in the second, the malignant epidemic form of the disease as it existed in Ludington. It is also apparent that in the endemic cases the plant whose activity has been developed within the patient may escape with the breath, and a second case of diphtheria be produced by contagion. It is also plain that as the plant gradually in such a case passes from the mild to the active state, there must be degrees of activity in the contagium, one case being more apt to give the disease than is another; also that the malignant diphtheria must be more contagious than the mild endemic cases. We think there is scarcely a practitioner who will not agree that clinical experience is in accord with these logical deductions from our experimentally determined premises.

It yet remains for us to investigate as to what are the conditions outside of the body which will especially favor the production of active micrococci, and also to study the effects of agents in killing these organisms; for it is very apparent that local treatment of the throat must often be of the utmost importance, and that it will be far more effective if it be of such character as to kill the micrococci, and not simply be anti-phlogistic in its action.

### SOLAR PARALLAX.

In an elaborate paper, given in full in the *American Journal of Science*, for November, Professor William Harkness draws the following conclusions:—

For convenience of reference the limiting values of the solar parallax, found by the various methods described in the foregoing pages, are presented here. It should be remarked, however, that in selecting these values the results of all discussions made prior to 1857 have been omitted; except in the case of the transit of 1761, and the smaller of the two values from the transit of 1769.

#### I.—Trigonometrical methods.

Mars, meridian observations .....	8".84	—	8".96
"    diurnal observations.....	8.60	—	8.79
Asteroids .....	8.76	—	8.88
Transit of Venus, 1761.....	8.49	—	10.10
"    "    1769.....	8.55	—	8.91
"    "    1874.....	8.76	—	8.85

#### II.—Gravitational methods.

Mass of the earth.....	8".87	±	0".07
Parallactic Inequality.....	8.78	—	8.91
Lunar Inequality.....	8.66	—	9.07

#### III.—Photo-tachymetrical methods.

Velocity and light equation.....	8".72	—	8".89
Velocity and Aberration.....	8.73	—	8.90

To obtain a definite value of the solar parallax, it would now be necessary to form equations of condition embodying the relations between the various elements involved; to weight these equations; and to solve for it by the method of least squares. But what is the use? It is perfectly evident that by adopting suitable weights, almost any value from 8".8 to 8".9 could be obtained, and no matter what the result actually was, it would always be open to a suspicion of having been cooked in the weighting. We only know that the parallax seems to lie between 8".75 and 8".90, and is probably about 8".85. Attack the problem as we will, the results cluster around this central value. All the methods give a probable error of about  $\pm 0".06$ , and no one of them seems to possess decided superiority over the others. We have nearly exhausted the powers of our instruments, and further advance can only be made at the cost of excessive labor.

In the beginning of the eighteenth century the uncertainty of the solar parallax was fully two seconds; now it is only about 0".15. To narrow it still further, we require a better knowledge of the masses of the earth and moon, of the moon's parallactic inequality, of the lunar equation of the earth, of the constants of nutation and aberration, of the velocity of light, and of the light equation. All these investigations can be carried on at any time, but there are others equally important which can only be prosecuted when the planets come into the requisite positions. Among the latter are observations of Mars when in opposition at its least distance from the earth, and transits of Venus.

In 1874 all astronomers hoped and believed that the transit of Venus which occurred in December of that year would give the solar parallax within 0".01. These hopes were doomed to disappointment, and now, when we are approaching the second transit of the pair, there is less enthusiasm than there was eight years ago. Nevertheless the astronomers of the twentieth century will not hold us guiltless if we neglect in any respect the transit of 1882. Observations of contacts will doubtless be made in abundance, but our efforts should not cease with them. We have seen that the probable error of a contact observation is  $\pm 0".15$ , that there may always be a doubt as to the phase observed, and that a passing cloud may cause the loss of the transit. On the other hand, the photographic method cannot be defeated by passing clouds, is not liable to any uncertainty of interpretation, seems to be free from systematic errors, and is so accurate that the result from a single negative has a probable error of only  $\pm 0".55$ . If the sun is visible for so much as fifteen minutes during the whole transit, thirty-two negatives can be taken, and they will give as accurate a result as the observation of both internal contacts. In view of these facts, can it be doubted that the photographic method offers as much accuracy as the contact method, and many more chances of success?

The transit of 1882 will not settle the value of the solar parallax, but it will contribute to that result, directly as a trigonometrical method, and indirectly through the gravitational methods with which the final solution of the problem must rest. As our knowledge of the earth's mass may be made to depend upon quantities which continually increase with the time, it will ultimately attain great exactness, and then the solar parallax will be known with the same exactness. Long before that happy day arrives the present generation of astronomers will have passed over to the silent majority, but not without the satisfaction of knowing that their labors will contribute to that fullness of knowledge which shall be the heritage of their successors.